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I find the doctrines of Rehmke and Michaltschew concerning the nature of consciousness, and the relation of consciousness and object, to be highly significant and important. Yet I am unable to see that these doctrines have any necessary relation to their 'immanental' point of view. Against this feature of Michaltschew's doctrine I can only repeat, in substance, the criticism urged against Rehmke. His aim is to start from a point of view which is free from presuppositions. But there is, and can be, no such point of view, at least for our human thinking. A presuppositionless point of view would imply a finally perfect and complete knowledge of self and the world. He finds such a starting-point, however, in the given. But what is the given? He himself can give no answer to this question without contradicting his own definition. For any given to which he may point is by his pointing made determinate. And no not-given can be even conceived. Either, then, his immanentalism is so broad as to mark no distinction whatever, or it contains, after all, an obscure implication of that subjectivism which he so heartily detests. That is to say, he cannot say what is given or not given except by setting up a world of ideas over against a world of things and then deciding, by contrast and cross-reference, what is included in the world of ideas alone — a world of ideas which is now separated from the world of things. His account of the given suggests, at different points of his argument, both of these criticisms. And in my opinion he is misled from the beginning by a false logic. Otherwise independent in his thinking, he has blindly accepted the traditional conception that the structure of knowledge must be built upon an independently solid ground. His conception of the given is then simply a fruitless attempt to satisfy this hypothetical demand.

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Le rationalisme comme hypothèse méthodologique. Par FRANCIS MAUGÉ. Paris, Félix Alcan, 1909. — pp. xii, 611.

A widely current view today is that science is a well-knit body of definitions; that proof, deduction, is a conceptual matter only; that intuition gives not universals but particulars; and that an *a priori* demonstration of objective scientific principles is at best doubtful. In sharp contrast to this seems the view of M. Maugé. His purpose is "determiner les conditions de l'expérimentation et de la systématisation dans les sciences" in a quite *a priori* way: "nous nous efforçons de déduire des idées d'expérience et de système les conditions de leur realisation" (p. i). Yet it is not limited to the conceptual realm, for we are enabled to derive "une logique de l'intuition que nous appliquons ensuite . . . tant à la découverte des lois naturelles qu'à celle des principes et d'un plan général de systématisation" (p. i). This, he admits, is only a demand, for there is no way of getting from the purely subjective to the objective; but it is a demand which enables us to choose properly from the mass of data those which suit the needs of science. Here he is able, at least in a negative fashion, to obtain an objectively valid clue which, as he believes, (pp. iii, iv) Kant's deduction was really unable to furnish. Without in the least claiming that we constitute and determine the objective world, or denying that all necessity is but a feeling, " un besoin affectif " (p. vi), he is yet able thus to get a criterion of external truth. As a defender of objectivity, he is also a partisan of intuition. Experiment is defined thus: " la méthode expérimentale consistera donc à isoler autant que possible les termes dont on étudie la relation: c'est en un mot une méthode intuitive *abstraite* . . . " (pp. vi–vii). The elements about which science is concerned are thus *not* concepts, but presentations; though abstract, yet really so and not ideal limits. Here is a revolutionary position; yet before judging it let us see how it is worked out in the book as a whole.

The system of M. Maugé is, we shall see, a most remarkable combination of apriorism with nominalism. But through it all the subjective character of the demand which created science is never lost sight of: though we can deduce *a priori* the form which science *ought* to assume, if there *is* any science, there might conceivably be no science. Things might be utterly irregular and confused. Thus M. Maugé's *a priori* rationalism loses none of its hypothetical character; it remains a " hypothèse méthodologique." Our problem now divides into two: analysis of the essence of systematic science, and the application of our result to the various sciences, to see that they do and can fulfil the *a priori* conditions which our analysis has discovered. These two problems occupy the first and second books respectively.

What then is the criterion by which we choose some sense-presentations as fit for the body of science and reject others as unfit? The first part of the deduction here is, we must confess, not what the plan and method of the book lead us to expect. Science is " un système de corrélations entre des modes de conscience exprimés par le langage " (pp. 32–33). Now language, words, form a discrete series; so do our ideas, being the inner correspondents of our words. This discontinuity in succession is just nothing but the form of time. Hence, time is a condition of the possibility of science (p. 35). It by no means follows that, as Kant thought, it is a condition of all experience. The author certainly regards this as a serious deduction: " la forme du temps se trouve ainsi déduite, comme imposée par les conditions de la possibilité de la science " (p. 35). Yet who can follow him here? Language is certainly a poor basis, with its irregular caprices of form and structure, whence to deduce the basis of the exact systems of mechanics and physics. It would surely be better to assume time at the outset. More logical is his second point, the deduction of the *constancy of law*. All statements, all reasoning, presuppose that what was said is true *after* its utterance, as it was *during* its utterance. This of course is only the old " A is A " in its modern form; the author seems not to be aware that the point is familiar. From this he leaps to the principle that *things* must retain their characteristics, display regular behavior under similar conditions. This principle, as Kant showed in his remarks about cinnabar being always red, is indeed a necessary one for science, but it is another principle than the above one, and needs either a reduction to the above one or a separate proof. It is, clearly, time that every bit of logical analysis like this should be done by exact and symbolic methods. There is however a further statement of this principle, sufficiently deduced by the author: namely, that the same causes must produce the same effects, if there is to be science (p. 39). The subjective correlate of this is, that one must be always able to recognize identity in objects. The reader will here note that the author's view of identity is only that of common sense. Any two objects may be identical, notwithstanding their individual distinctness.[1] From what we have deduced it follows that those elements, given in experience, which are recognizable as the same, which display unchanged characters in all their permutations and combinations are the only ones fit to be incorporated into science (p. 47). Now this is just what experimental science and inductive methods bear out. Mill's five methods attempt to discover permanent constant elements. Incidentally it follows that *causality* is not a universally necessary principle: every change must when repeated be accompanied by the same antecedents and consequents, but this is only on condition that there be science, and even then applies only to changes. This is of course a thought not unfamiliar to idealistic philosophers, who regard causality as valid only from a certain point of view. But the main interest of this analysis of the experimental method (Chapter II of Book I) lies in its criticism of the current induction. This method is one of elimination: " elle élimine *idéalement* les coïncidences fortuites, et elle declare que les causes cherchées sont constituées par les résidus " (p. 70). That is good for discovery, not for knowledge. Experimental proof of a cause is found only in isolation. This is one of the novelties of the book; and in the reviewer's opinion perhaps its greatest merit. There should be direct observation of the cause, acting alone, followed by its effect.

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[1] Cf. p. 146 " Deux choses qui ne diffèrent que par de rapport extérieur de position sont, prises en elles mêmes, une seule et même chose et leurs propriétés sont indiscernables."

This " isolation," or " material abstraction " is the nerve of all real science. It is done (p. 71) by preventing other agents from coming into the experiment — as in making a body fall in a vacuum, " suppressing " the disturbing agent of atmospheric resistance in order to see gravitation acting in isolation; by " segregation " or isolating a chemical substance in a glass; " neutralization " where the two above are materially impossible and we estimate a cause by seeing how much it takes to neutralize its effect; and finally " differentiation " where the disturbing agents are reduced to a negligible amount — as in the pull of remote stars upon the earth (pp. 72–73). " Ainsi la méthode expérimentale est une méthode d'abstraction matérielle." And what terms can be represented most usefully and clearly in abstraction? Color without form not at all; form without color easily.

One more point and the first book is in essence finished. Since a cause is intuitable, it cannot be a universal. Whence then the generality of scientific laws? Simply from the fact that the *utterly* isolated cause — the mere falling body without further determinations — is so indeterminate as to permit indefinite range of application — in short is practically itself a universal. Of course this is bad logic; the vague is not essentially any more universal than the well-defined. Here as in several other places the author ignores many of the results of the history of philosophy. His system is undoubtedly a mechanical mixture, not a chemical combination, of apriorism and nominalism. And yet we cannot but admire the convincing account of isolation as the core of experiment. It certainly seems to the reviewer a sound point. And there is too, we think, an individuality about causes which present theories of science tend to overlook. How to harmonize these nominalistic motives with a healthy apriorism will doubtless long be the great problem here. We do not believe M. Maugé has quite succeeded; but he has brought out the issue clearly, and has strengthened the nominalistic side.

The aim of the Second Book is to show that while the theory of scientific method just given is in some of the less exact sciences inadequate, the gaps may be filled in. To do this it gives in Part I a careful account of the proposed method: the first chapter explaining in detail ' synthetic deduction,' and the second showing how it is made possible by the very structure of experience and of science. In Ch. I we learn that the old syllogistic logic is of no use, as it does not enlarge our knowledge; so a new kind is needed. This is " synthetic deduction," which reduces to " une equation entre un fait isolé et ce même fait, compris dans un groupe d'où la pensée l'extrait par l'analyse " (p. 168). This " fait isolé " is some one privileged case, not itself materially abstracted as yet — that is why this is deduction and not induction. Such a case is the straight line in geometry, the atom in chemistry, the electron in physics. This kind of deduction is quite hypothetical, telling us only what would happen in case such and such a complex of these atomic elements did occur. But with the progress of science these elements ought to be verified, — they are not mere " convenient fictions " (p. 180). Now the sciences as actually existing are built up largely out of these synthetic deductions: many of the deductions start from real materially abstracted elements, and many from elements which have not yet been isolated. Besides the above there are a few local excursions in this chapter, which as a whole seems to the reviewer rather unsystematic and confused: *e. g.*, on causation, (pp. 176–177), definition of the notion (pp. 184–185), and the principles of composition, importation, etc. (p. 195). Ch. II shows how experience furnishes us one very convenient element, in terms of which we can express all or an increasing number of the external facts, namely, kinesthetic sensations (p. 205). Hence science would reduce all to motion: *that* is " le langage le plus propre à traduire les relations objectives résultant de la nature des choses " (p. 208). In general, the scientific element must be simple and definite and such that its properties are independent of the size or complexity of the object wherein it exists. Cases of these, reducible of course at bottom to motions, are material points (p. 235) as " éléments gravifiques," electrons (p. 238), and ether particles (p. 242).

Part II of Book II carries out the program in the specially difficult and complicated fields of biology and psychology. The discussion is certainly careful and sustained, occupying a little over 300 pages. Of the merits of his program for a complete synthetically deduced science of biology we are unable to judge; it seems a question not for the philosopher but for the biologist. M. Maugé is an extreme mechanist, taking as physiological unit assimilation, with its complementary process of cell-division (p. 273), reducible theoretically at least to a more elementary unit, the plastid (p. 318). He admits however at the end of Ch. I that deduction cannot at present carry its designs very far (p. 371). The discussion of psychology does not, we must admit, seem very convincing. He distinguishes the psychical from the physical thus : " les mêmes représentations apparaissant tour à tour comme objectives ou comme subjectives, suivant qu'elles sont considérées comme déterminant dans tous les esprits des réactions identiques ou des réactions diverses . . . " (p. 372). And again, " un phénomène est objectif . . . quand il réalise les conditions qui pour tout esprit normal, rendent cet accord possible, . . . il est subjectif, dans la mesure où l'impression qu'il laisse, varie avec les individus qui en sont témoins " (p. 377). How then, we might ask, can psychology be a science?

Where is the verification possible, on this definition? The author seems not to know this well-worn objection. The psychic atom or element, corresponding to the biological assimilation, is *affective tendency*. All psychical states may, it seems, eventually be stated in terms of these tendencies, combining, clashing, separating, etc. (pp. 423–464). Certainly the proposal is revolutionary enough to need a separate treatise. Can it be wise to make one's philosophy of scientific method depend on a thesis, to prove which one must first turn a certain science quite around?

Much of the discussion is taken up with the question of the relation of body and mind. The author is a parallelist (p. 390) on apriori grounds. The question whether or no there is quantity in mental states is decided in the affirmative (p. 415). The physiological equivalent of representation is "ionization" (cf. pp. 485–487). The ideal would be, by combining hypothetically the psychical elements, to deduce what kinds of states a conscious subject could have. The author tries a little of this (pp. 499–506); but it is hardly possible to criticise such an attempt adequately in passing. The last division but one of the book is entitled "Psychogénie" (p. 528) and has for its aim the synthetic deduction of the genetic side of psychology: of perception, conception, memory, imagination (pp. 528–562). It does not seem advisable for anybody to undertake so huge problems as the author here treats merely by the way. The whole of genetic psychology, the genesis of social modes (pp. 562–590), the transformation of psychology, and a deductive biology — these subjects must be, even in the barest outline, the work of many men at many different times. On the other hand, the general thesis of M. Maugé may perfectly well be defended as an ideal of science without one's proving just how in detail it might be worked out. We believe that its strength lies in the insistence on the intuitive side of science, and at the same time claiming for the apriori aspect an equal recognition. It is a splendid attempt on a very large scale to reconcile these two opposing tendencies of thought. Its greatest value, in the reviewer's opinion, is in the motive of inclusiveness which seems to dominate the author's work. At the end (pp. 591–606) is an excellent summary of the general philosophical position, clearing up many points which taken by themselves are a little obscure. On the whole, the style is rather difficult.

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Old Criticism and New Pragmatism. By J. M. O'SULLIVAN. Dublin. M. H. Gill & Son, 1909. — pp. xiii, 317.

This book consists of four essays. The first, which was presented to the faculty of the University of Heidelberg as a doctor's dissertation and